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ABSTRACT

Research to date has provided too few answers for vital educational questions concerning teaching children or letting them learn. A basic problem is that experimentation usually begins by accepting conventional assumptions about schooling, ignoring experiments that would entail disturbing the ordering of current educational priorities. Researchers' own conservatism is also an obstacle to decisive research. It is proposed that a proper function for the educational research community is to debate the vital issues of education, free of the constraints of present assumptions and sustained over long periods of time. One strategy for gaining verifiable convictions is illustrated in a case study which involves engaging in a program of empirical research in cognitive development and in a systematic reexamination of the available research literature. Five features are critical: cumulative, developmental, experimental, comparative, and realistic research. The essence of the strategy is that basic research can be used to generate instructional hypotheses that are believable enough to gain public support for decisive research involving direct experimentation with instruction. The only way to find out what is teachable is by discovering how and when it can be learned with ease. (LH)

Children and Adolescents: Should We Teach Them or Let Them Learn?[1]

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We are largely ignorant when it comes to answering questions about vital issues in education. What should be taught? When should it be taught? How should it be taught? To whom should it be taught? What are the consequences of teaching this rather than that, of teaching it now rather than then, of teaching it this way rather than that way, to this person as well as to that person? If candid, our professional response to these questions is that we don't know. Indeed, we might even take some measure of pride in this response. As researchers, it is fitting to disclaim possession of final answers and to advertise the boundaries of our knowledge. Thus, we can justly enjoy good feelings about declaring the modesty of our wisdom. But whatever comfort we derive from this honest statement of our limitations, it cannot last long because we are also committed to making significant reductions in our ignorance.

If you find this appraisal too extreme to deserve consideration, imagine yourself in the role of a trusted and respected consultant, endowed with all of the most powerful theory, evidence, information and skills that we share collectively. Suppose a client comes to you with the questions just mentioned for the purpose of implementing your answers by establishing an entirely new system of schooling. Assume the client has all of the resources necessary for accomplishing this purpose excepting only the information he seeks from you. To inject a note of realism, let him impose two conditions: that the system should serve a population having a composition comparable to that in the United States; and that the maximum time he can allow you to answer the questions is ten years. Also suppose the client offers to fund your efforts to obtain answers, on whatever scale necessary, for the ten-year period. In advance, he requires only two things of you: that you explain the methods you propose to use, showing their potential for answering the questions; and that you give him your personal assurance that the probabability of your success is reasonably high. Suppose that success simply means obtaining enough answers to yield some concrete proposals for a program of schooling.

Could you comply? I am not confident that I could. And the reasons for my pessimism do not seem trivial--they do not concern shortages of trained personnel, nor an inability to estimate costs, nor a fear that an interval of ten years is too short, even though it probably is. The reason is that I note precious little evidence that questions of the kind asked by the client are the questions typically addressed by educational research.

If this appraisal is accurate, it is dismaying because it seems to me that the questions posed by this client are important questions. They are obviously the same questions that are being asked currently about schooling in the United States. The imaginary client, after all, is not a product of pure fantasy. He could easily be a district superintendant, a school board chairman, or, more likely, a visible representative of the public--a community leader or a legislator. In fact,

educational researchers are also fascinated by the issues the questions raise. Thus, it is tempting to wonder why the prospects for obtaining answers are not brighter.

The Problem of Conducting Decisive Research

I will mention only two of the many possible reasons for the fact that research to date has produced too few answers for vital educational questions. One reason is that it may be practically impossible to do decisive research in education. Another reason is that educational researchers may be too conventional or conservative to engage in it. The first reason is especially important to those who wish to conduct research that is experimental in character. Questions about when, how, and to whom topics should be taught seem to demand the manipulation of different schooling programs so that their effects can be compared. At least the questions require that variations in schooling programs and procedures be available in nature even if they cannot be manipulated. The argument is that decisive research entails experimentation with a system, education, that is as fixed as the solar system, and therefore open only to the methods of observational science.

A fair retort to this argument is that it ignores available facts. After all, many aspects of education have been subjected to experimental analysis. Examples abound, but the subjects of reading and mathematics are good ones. Numerous studies have manipulated instructional methods for assisting children to acquire reading skills and the results give comparatively persuasive answers about the relative merits of the methods. The advent of the "new math" has spurred many experimental contrasts of methods for teaching mathematics and such research has provided some answers. As in any field, research in the areas of reading and mathematics is of uneven quality but the solid studies have been relatively conclusive. So, what is the insurmountable practical problem alluded to?

The problem is that virtually every instance of an experimental attack on an educational issue starts by accepting conventional assumptions about schooling. How many studies are there that have addressed the question whether formal reading instruction is more effective if it begins at age ten than at age six? What would result if mathematics instruction were scheduled only during alternate years, beginning in the second grade? How would an elementary school program designed to improve interpersonal skills affect the acquisition of traditional academic skills? Do children derive more profit per year of formal instruction when they are five or when they are thirteen? Questions like these make the practical obstacles visible because the experiments they entail would disturb the ordering of current educational priorities.

Questions very similar to these have spawned experimental research but only when they could be answered without challenging the present criteria of schooling. We are now rich with data about the effects of a variety of types of early childhood education. Academic, cognitive, traditional, one-year and two-year preschool programs have been compared with control groups and with one another. We even have some evidence about the comparative effects of different programs for different populations of children. But note two features of the studies that have provided us with this kind of evidence. One is that they all involve adding years and programs to the present structure of schooling, not deleting any from it or displacing any within it. A second feature is that the success of the various programs is mainly judged by their effects on the usual criteria of schooling--do the children read better by the end of first grade, or do they score better on IQ tests by virtue of participation in the preschool program. Thus, we are free to manipulate by experimenting with pre-first-grade programs, and probably even with post-twelfth-grade programs. But the intervening years remain largely inviolate. This is the practical obstacle.

The reality of this barrier must be admitted. Nevertheless, it need not intimidate us. It serves a useful function: to moderate runaway, and often unjustified, revisionism. Shortly, I will suggest that the barrier simply requires us to demonstrate, convincingly, that major variations in the structure of established schooling promise to improve its outcomes. If we cannot do this, we are on extremely shaky ethical grounds anyway and deserve our bonds.

This discussion of the practical constraints on progress in educational research has already touched on a second obstacle to decisive research--our own conservatism. Before trying to construct a brief for this part of the case, let me moderate my conclusion in advance. The conclusion will be that our research bears the mark of deference to conventional beliefs, and commitment to the preservation of customary practices. The qualification is that there are at least three major functions for educational research and the conclusion applies to only one of these.

The educational research community is already performing rather well in improving instructional methods and programs for attaining currently prescribed objectives of schooling. Evidence that this function is being fulfilled and that additional progress should be anticipated can be found in a number of examples (see, for example, Beck & Mitroff, 1972; Resnick, Wang, & Kaplan, 1970; Weikart, 1971). These examples are noteworthy for the degree of success achieved, the conception that has guided them, and for the fact that they have been attentive to the needs of students who vary widely in background characteristics such as developmental rate, socioeconomic status (SES) and ethnicity.

Another research function is also represented visibly in the work of our community. Drawing on the discipline of differential psychology, many of our members persistently pursue the sources of individual differences in educational outcomes. At present, for example, we are witness to a fairly lively set of controversies that have centered on a publication in the Harvard Educational Review by Arthur Jensen (1969). Judging by the number and variety of persons who have gotten into this act, we are deeply involved in the task of accounting for variation in school success when the criteria are the customary ones of performance on IQ and school achievement tests.

Unfortunately, a third potential function of educational research is not adequately realized . This function may be described as that of fostering change in the basic framework of schooling. It seems proper for the research community to question present assumptions, propose alternative ones, provide a theoretical and empirical rationale for the alternatives, demonstrate the promise of those assumptions, and evaluate the consequences of implementing the assumptions. Bluntly, we ought to engage in a sustained and effective challenge to the status quo. Proposals for radical change in schooling may be plentiful enough to be regarded as constituting a movement, but if so, the educational research community is not in the vanguard.

Let me try to be clear on this point. I am not suggesting that we are obliged to excel at the kind of rhetoric and the kind of radicalism that pervades the semi-popular educational literature. Instead, I am proposing that a proper function for the educational research community is to debate the vital issues of education, free of the constraints of present assumptions. Moreover, such discussion should be sustained over long periods so that it can result in research designed to answer the questions raised by the issues.

As I see it, the main obstacle to progress in this regard is that too many of us, either tacitly or explicitly, accept the validity of present systems of schooling. We capitulate in the use of end-of-first-grade reading tests to evaluate the effectiveness of early childhood programs. When disproportionate numbers of children from poor homes fail in school, we may complain that instruction is inadequate, but what we do, at least visibly, is to ascribe the failure to the child's disadvantage. We recommend that he enter school earlier to compensate for the disadvantage. Or we control for his disadvantage by showing that he succeeds as well as can be expected given his low IQ. Either way, the present criteria and procedures of instruction remain unscathed.

Perhaps we hesitate to challenge the validity of the current frameworks of schooling because of the weight of supporting evidence. Perhaps, but probably not. We know as well or better than others that most of the evidence is far from compelling. Elsewhere (Rohwer, 1971) I have argued that one of the chief kinds of evidence, the relationship between educational attainment and occupational placement, may be freely regarded with great skepticism. To be sure, the relationship is a strong one but it may be artifactual. The reason is that educational attainment determines the level of entry into occupational hierarchies. Within such levels, the relationship between school performance and occupational proficiency is marginal at best. Most other ways of demonstrating the validity of schooling are equally suspect. The relationship between school performance and IQ, for example, is patently circular. Thus, the evidence is not sufficient to inhibit us. We are free to question existing frameworks and proceed from there.

Such questioning is already evident in recent statements of some educational researchers. Although I disagree with his formulation, one instance is provided by Jensen's (1969, 1971) conclusion that a major alternative form of instruction must be developed. Another example may be found in Bereiter's (1969) proposal that education be abolished in favor of teaching only those skills that can, in fact, be taught successfully to virtually all children. Glaser (1972) has presented a persuasive brief for displacing the selective model that currently controls the structure of schooling with an adaptive model. Encouraging instances like these are no longer hard to come by. But, until we can sustain the discussion, a coherent body of radical theory and research will not emerge. So far, we have not done so.

At present, too little of our research is decisive for questions of what should be taught, when it should be taught, and for estimating the consequences of answering the questions one way rather than another. For example, consider Humphreys' (1971) recent assertions that, "...Subject-matter knowledge or skills in these areas [arithmetic, history, science] is not a necessary, perhaps not even a useful, goal for the first six grades. ...Formal teaching, or learning of arithmetic can be delayed until the seventh grade, and all of the learning required for starting high school mathematics can be accomplished by the end of the eighth grade... By the end of the sixth grade it is better that a child like science than that he know a great deal about science." There are three or four different assertions in this single quotation, every one important in the sense that it implies a drastic change in schooling. But I am unable to find any data that can be used to verify the assertions, one way or the other.

To take another example, entertain the hypothesis that children would become more skillful readers if reading instruction began at age nine or ten rather than at five or six. At present we have no data that are directly relevant to the hypothesis; the necessary research has not been done. We are beginning to obtain evidence about the effects of starting reading instruction earlier than usual but there is no indication on the horizon that we will ever learn about the consequences of starting it later than usual. We know that the earlier in school a child becomes an accomplished reader, the better his chances for succeeding in other school subjects. For all we know, however, this may be due entirely to the fact that instruction in other subjects relies so heavily on printed educational materials.

Another variation on this theme emerges in our tactics for dealing with individual differences. If there is reason to suspect that a child will encounter substantial difficulty in learning to read, we prescribe a larger dose of instruction, at an earlier age, than for a child who is likely to learn with ease. The rationale for writing this prescription, however, is no more compelling than for another one--that the child should receive reading instruction at a later age. How are we to choose between these two prescriptions? Properly designed research studies could provide a basis for choice, but they have not been conducted.

On an issue like this one, that is, the optimal timing for instruction, our research does not appear to be relevant. If this is true, it is a clear and present obstacle to progress.

It is easy to conceive of research that would be decisive for vital questions about education. Genius is not required to design studies that would resolve issues such as the proper timing of instruction or tailoring instructional timing to individual differences. The problem is that such studies appear to risk the survival of the children who might participate in them. Parents, educators, legislators accurately perceive that failure in school drastically limits the range of choices available to those who fail. Thus, they would be right to oppose many conceivable experimental studies of education. Imagine a study, for example, that included a comparison of different ages for the onset of reading instruction, say age ten versus age six, for a representative sample of children from rich homes and another of children from poor homes. The consternation this proposal would arouse in concerned parties would be perfectly understandable.

Dozens of other examples of decisive research studies could easily be constructed. But the exercise is pointless unless there is some means of gaining the cooperation necessary to conduct the studies. If we wish to conduct decisive research we have no choice but to deal with the practical obstacles that bar the way.

An Avenue to Decisive Research

Consider two research objectives: (a) determining the optimal time for beginning formal instruction about some skill or topic, and (b) determining how this schedule should be varied in order to adapt to individual differences. To accomplish these objectives in terms that bear directly on specific school subjects such as reading and arithmetic, research studies must eventually include tasks drawn from those subjects. Because of practical obstacles, however, we cannot begin with such studies. We must first find ways of providing assurance that children who participate in them stand to benefit more than they stand to lose. And to give assurance, we need to have some verifiable convictions about the development of mental processes in children and about the role of instruction in activating those processes. I should like to address this matter by means of a case study that illustrates one strategy for gaining verifiable convictions. This strategy involves engaging in a program of empirical research in cognitive development and in a systematic reexamination of the available research literature in that domain.

The research program to be described is characterized by five features considered to be critical for opening a path to the resolution of fundamental educational issues. These critical features may be summarized as follows:

1. Cumulative Research. The program of research should be planned so that the same kind of task may be used repeatedly. The reason for this is that it serves an intent to specify the underlying process responsible for observable performances. If a variety of tasks are to be used, there should be at a minimum, clear reason to believe that the same process is being tapped from task to task.

2. Developmental Research. Given the first feature, studies should examine task performance across as wide a developmental range as possible. The purpose, of course, is to detect performance changes that might signal important developmental shifts in underlying processes.

3. Experimental Research. Because the ultimate aim of the program concerns instruction, we need to know the effects of various experimental conditions on performance. In particular, we need to know the identity of conditions that produce optimal performance on the tasks used or that optimally activate the processes responsible for that performance.

4. Comparative Research. Assuming that the research program is cumulative, developmental and experimental, it should also be comparative because we want to make inferences about individual differences. That is, we need information about the range of variation across different kinds of children that is associated with the effects of different experimental conditions at different ages.

5. Realistic Research. Finally, the proposed strategy is based on a key assumption: the optimal timing of formal instruction will be indicated

by evidence that the underlying process necessary for successful task performance is available in a substantial proportion of the population. In other words, we are not seeking ways of making children precocious but ways of assisting them to be optimally effective.

The case illustration is a research program concerned with the development of learning processes in children and adolescents. It will be described in terms of several steps to highlight the principal components of the proposed strategy for developing a means to conduct decisive research. To be presented first is a description of the task most often used in the research program. Then the assumptions that launched the program will be enumerated and contrasted with the conclusions that have resulted from it. Next, an account will be given of the effort made to specify the underlying process responsible for task performance. The process description will then be used to characterize developmental and individual differences in task performance. Finally, a process of deriving instructional hypotheses will be illustrated. The essence of the proposed strategy is that basic research can be used to generate instructional hypotheses that are believable enough to gain public support for decisive research, research involving direct experimentation with instruction.

The Task

The task typically used in the illustrative research program involves presenting to the student a list of paired words, such as BAT-CUP and ARROW-GLASSES, directing him to study the pairs, then testing him by presenting one word from each pair and asking that he supply the missing words. Such paired-associate tasks are often used in laboratory research on human verbal learning and memory. In recent years, they have also appeared in a number of studies designed to analyze the development of learning processes in children. Even though they are ostensibly simple, paired-associate tasks appear to involve porcesses that are common to other, more complicated tasks. For example, significant correlations have been observed between paired-associate performance and reading achievement in the early primary grades (Lambert, 1970; Rohwer & Levin, 1971) as well as with IQ test performance at several age levels: kindergarten through grade three (Rohwer, Ammon, Suzuki & Levin, 1971); grade six (Rohwer, 1966); and grade twelve. Moreover, when college students are interviewed after learning a list of paired-associates, they report using a variety of strategies to master the task. These two lines of evidence imply that the task of paired-associates taps processes that are conceptual in character and that are more obviously relevant to schooling. On the other hand, students do not commonly receive direct instruction in how to learn associations between unrelated pairs of words. Thus the task is a handy one for the purpose of analyzing the development of learning processes and for determining the effects of instruction on performance. It draws on processes implicated in at least some school subject learning, but it is relatively free of the effects of within-school instruction.

Original Assumptions and Recent Conclusions

The early stages of our work were guided by several assumptions (cf., Rohwer, 1967, 1968). It is instructive to state these in order to make clear the manner in which they were forcibly altered by the kind of inquiry proposed. Three of the assumptions are of particular interest.

a. Several different underlying processes are responsible for performance on the task. These processes vary in terms of modality--verbal versus imagery--and in complexity--rote vs. conceptual. Thus, efficient performance was thought to result from one kind of process and inefficient performance from another.

b. Developmentally, the shift from inefficient processes to efficient ones was presumed to occur commonly over the age range four to seven years.

c. In this age range, individual differences in other kinds of learning, learning to read, for example, were assumed to be related to individual differences in the efficiency of the process whereby children learn paired associates. Thus, it was predicted that low-SES students would perform more poorly than high-SES students on paired-associate tasks. Furthermore, it was expected that special instruction in techniques for learning, especially if given during this early age range, would assist low-SES children to perform as well as high-SES children.

At present, no one of these assumptions seems tenable (Rohwer, in press). Instead, the facts call for substituting three very different assumptions.

a. A single underlying process, which I call <u>elaboration</u>, is responsible when paired associates are learned. Therefore, the efficiency of performance is determined by whether or not the elaboration process is triggered.

b. Developmental changes in paired-associate performance are not caused by changes in the character of the underlying process. Rather, they reflect changes in the conditions required to trigger the single process. The process itself is available from at least age four on. But there are three important developmental shifts in the kinds of conditions required to activate elaboration. Only one of these typically occurs in the age range four to seven years; another averages between seven and nine; and the other appears much later, usually between twelve and sixteen years.

c. There are individual differences in the conditions required to activate elaboration. But, during childhood, that is, from approximately four to twelve years, these differences relate little, if at all, to classifications like SES or ethnicity. The average age of the developmental shift during childhood appears relatively constant across a number of different populations. In contrast, the later developmental shift does appear to be related to SES, it appears to emerge earlier for high-SES than for low-SES students. Even so, it can be demonstrated convincingly that the process of elaboration is available in virtually everyone, regardless of group membership or within-group individual differences.

Let me try to indicate what forced the change in assumptions and draw out some implications of the changes. Before doing so, however, it is important to mention one feature of our approach that has not changed over the last ten years. That is, a prominent research objective has been to discover conditions that produce optimal performance on the task. In this sense, the approach is directly oriented to instructional considerations, that is, to the question how best to assist students in achieving mastery.

Specifying an Underlying Process

Given the production of optimal performance as a criterion, the original first assumption was readily discredited. Studies began to show that optimal performance on a paired-associate task could be achieved by virtually all subjects, regardless of age or background, depending only on the implementation of the proper task conditions. In one study, for example (Irwin, 1971) the use of peculiarly effective learning conditions produced optimal performance in five year olds that was equivalent to that observed in eleven year olds. (And the outcome was not produced by an artificial ceiling effect.) It is worth mentioning the conditions necessary for this result. All children were asked to learn a list of 32 noun pairs. The nouns were not just presented as words, however; each one was represented by an object it denoted. Furthermore, as the experimenter displayed a pair of objects, he enacted an episode involving them. In the case of BAT-CUP, for example, he placed the handle end of the bat in the cup. After one presentation of the pairs in this manner, the five year olds, as well as the eleven year olds, scored approximately 81% correct responses on the average, an impressive level of performance indeed (cf., Wolff & Levin, in press).

In order to specify an underlying process, however, it is not enough to know the conditions sufficient for optimal task performance. It is also important to identify conditions in which optimal performance does not occur. This has been done using the task just described: the learning of noun pairs. If the pairs are represented only by orally presented words and if the only assistance provided by the experimenter is the instruction to learn the pairs, the average performance of groups of five year olds will be very poor. After a single presentation of the list, less than 10% of their responses will be correct. In fact, when the task is presented this way, available research suggests that optimal performance is achieved only by students who obtain high scores on IQ tests and even then not until these students are between fourteen and seventeen years of age. Thus, the two versions of this single task yield results that are markedly discrepant.

The question of interest is how to account for the discrepancy. One way is to assume that different processes are at work in the two versions of the task. A related assumption would be that one of these processes is available to all persons, including five year olds, while the other is available only to high-IQ persons beyond the age of sixteen. Some may wish to maintain such a hypothesis but I find it uncongenial for two reasons. First, it seems needlessly complicated. More important, however, it seems unreasonable to believe that two distinctly different processes are involved when (a) the task itself remains the same (coupling together unrelated pairs of nouns) and (b) optimal performance can be produced simply by arranging the proper conditions for learning. Thus, I much prefer the hypothesis that performance on the task is determined by whether or not the one necessary underlying process is activated, rather than by which of several different underlying processes is activated.

Suppose we adopt the position that a single conceptual process is responsible for the production of correct responses on noun-pair tasks. It still remains to find a way of explaining the discrepant results obtained with different versions of the task, and also of explaining the discrepant results observed across different individuals. To accomplish this, it is probably useful to construct a more detailed description of the character of the hypothesized underlying process. Start with the end-product of the process, namely, the coupling together of pairs of items that were initially unrelated. Assume that this coupling is achieved by generating an event that integrally involves both of the items. Call this process, <u>elaboration</u>. That is, the elaboration process consists of generating an event that serves as a common referent for the items to be coupled. The term event refers to an episode that integrates one or more objects or actors into a common relationship. When this process is activated for any pair of items, the items will be learned, when it is not activated, the pair will not be learned. The key for explaining discrepant results across versions of the task and across varieties of individual then, lies in the triggering conditions necessary to activate the process of elaboration.

A review of the research literature in this area suggests that triggering conditions can be ordered in terms of how explicitly they prompt the subject to generate referential events for the items to be learned. From study to study, task conditions vary with respect to how explicitly they prompt elaboration, and persons vary in how explicit the prompt must be to elicit elaboration.

Further analysis suggests that we can account for most of the reported phenomena by distinguishing four degrees of prompt explicitness: maximal, substantial, moderate, and minimal. At the maximal level, the task conditions include an actual demonstration of an event to be generated for each pair of items to be coupled. A substantial prompt involves presenting a description or representation, either by means of words or pictures, of an event for each pair. A moderate prompt consists of directing the subject to think of an event that integrates the pairs of items. And a minimal prompt simply instructs the subject to learn and remember the pairs.

The Elaboration Process, Developmental Trends, and Individual Differences

It is now possible to see how this analysis can be used to describe developmental and individual differences in performance on noun-pair tasks. The displays in Figure 1 are a useful prompt for this purpose.

Insert Figure 1 about here

The picture presented there is an idealization of actual data, but on the whole it only smooths out reality rather than violating it. The columns at each age level represent groups of students from high-SES backgrounds (upper panel) as well as groups of students from low-SES backgrounds (lower panel). Note that at every age level for both these populations at least one of the columns reaches the hypothetical level designated as "Optimal Performance." The different kinds of shading within the columns indicate the explicitness of the prompts required to produce that level of performance. At the earliest age, for example, maximal prompts, the solid columns, are essential for optimal performance. By about age 7, the criterion can nearly be met with substantially explicit prompts, represented by striped columns. By age 9, continuing through age 12, moderately explicit prompts (stippled columns) are almost sufficient. Note too that through age 12, the picture for low-SES students is identical, in all essential respects with that for high-SES students. Thus, there is no evidence here to support the claim that the two populations differ, in the childhood age range, either in the kind of underlying process responsible for optimal performance nor in the reactivity of the process to various kinds of prompts.

It can be seen that the relative effectiveness of maximal, substantial and moderate prompts remains fairly constant across the range from 12 to 18 years. For high-SES students, the marked shift during this period is in minimal prompts. By the end of the period, minimal prompts are sufficient for optimal performance whereas at the beginning of the period, at least moderately explicit prompts are required. The general trend across the entire range, from early childhood to adulthood, is characterized by marked increases in the extent to which the elaboration process is self-activating. At the beginning, there is virtually complete dependence on maximally explicit prompts by the end, there is almost complete autonomy from explicit prompts.

In the adolescent age range, a substantial between-groups difference is evident in the effectiveness of minimal prompts. The shift for the high-SES group contrasts sharply with the continuing insufficiency of minimal prompts for the low-SES group, even after age 12. This difference in the average developmental pattern for the two populations appears to be quite reliable. The interpretation of the difference, however, is disputable.

I regard as insupportable the claim that differences in underlying capability are responsible for the discrepant effectiveness of minimal prompts for the two populations, since it is too difficult to square this interpretation with the fact that only a moderate prompt--the suggestion to the subject that he generate an event--is necessary to produce optimal performance. The equivalent effectiveness of such moderate prompts seems to me to demonstrate that the underlying capability is distributed equally in both populations. Thus, rather than stemming from a difference in underlying process, the source of the phenomenon appears to be that members of one group have acquired a propensity for autonomous activation of the elaboration process while members of the other group have not learned this skill.

The facts here are reminiscent of those produced by research using tasks that allegedly tap formal operations in the Piagetian sense. Analagous differences among populations led Goodnow (1971) to conclude that the discrepancies are explainable in terms of whether or not "tricks of the trade" have been learned for dealing with the problems presented by the tasks. Similarly, the interpretation I am suggesting for the results of research on noun-pair learning asserts that many persons do not learn the trick of triggering the elaboration process whenever they are confronted by tasks demanding that they couple together disparate items of information.

Deriving Instructional Hypotheses

It is obviously premature to base definite instructional recommendations on the results and interpretation of a single research program. Nevertheless this case study can be used to illustrate the process of deriving instructional hypotheses from basic research. Let me reiterate that the goal of the proposed strategy is not to establish a direct link from basic research to educational change. Instead, it is to use basic research to demonstrate the promise of particular changes so that decisive research can be done to directly evaluate their effects.

In thinking about the implications of research on noun-pair learning, it will be helpful to look again at the summary provided in Figure 1. One of the prominent implications suggested by this display is that a distinction be made between two kinds of learning: the learning of specific content on the one hand and the learning of skills useful for acquiring specific content on the other. With regard to the learning of specific content, the implications of the research seem unequivocal. If the content is important enough that we require children to learn it, we are obliged to design instruction that will make learning optimal. For some children, younger children for example, instructional prompts must be more explicit than for other children. But, according to the information in the Figure, we can discharge the obligation for virtually all children, regardless of age or background. That is, for both populations, at every age level shown, some of the columns invariably reach the level of optimal performance; in every case, the students have mastered the content with ease and efficiency.

Consider an example pertinent to this point about content learning. Recently Robert Matz and I conducted an experiment that tests the generality of the elaboration interpretation of noun-pair learning. In this study, the task was to listen to three short passages of expository prose and to answer eight true-false questions about each passage. We viewed the task as requiring the listener to generate events as common referents for the information in the passages, that is, we assumed that an elaboration process was involved in performing the task. Accordingly, we manipulated the kind of prompt presented along with the oral rendition of the passages that was given to all the children. In one condition, the printed text of the passage was presented as it was read aloud to the child. In the other condition, the information in each passage was represented pictorially as the prose was read aloud to the child. Since we believe that the elaboration process is more easily triggered in many children by pictorial rather than printed prompts, we predicted that performance would be better in the pictorial condition. Note, however, that with regard to content learning, the task was identical in both prompt conditions, namely, to comprehend and remember the information in the passages.

We sampled subjects from two populations of fourth-grade children: one from a school serving a high-SES white community, and the other from a school serving a low-SES black community. We knew in advance that the two groups differed by about two grade levels in terms of performance on standardized tests of reading achievement. So, we took the precaution of including in the passages only words that could be accurately identified in their printed form by children from both populations.

The results corroborated our predictions. In the printed text condition, the high-SES white sample produced an average of 82% correct responses; in the pictorial condition, the average was 90%. The difference between the two prompt conditions was even more dramatic for the low-SES black children: the text prompt produced only 58% correct responses—a very low level of performance considering the fact that chance level on the true-false test was 50% correct. In the pictorial condition, however, a near-optimal level of performance was achieved: an average of 81% correct responses.

Thus, on this task of comprehending prose passages, mastery of the specific content was assured, for both samples, by establishing effective prompt conditions. Had this been a real task, that is, one required of the child in school, the instructional obligation would be to present the content so that it could be learned easily and efficiently. This much is clear. For if the content were critical for subsequent learning, say for participating in an exercise in science observation, improper presentation would effectively deny many children the opportunity to profit from the next experience.

Besides illustrating implications for content learning, the outcome of this study also emphasizes the importance of skill learning. Especially in the sample of low-SES black children, the skill of activating elaboration given a printed text prompt--in other words the skill of reading--had not been successfully acquired. Does basic research in cognitive development have implications for instruction in the acquisition of skills?

Looking at Figure 1 again, it appears the answer is "Yes." Keep in mind that in the elaboration model a skill consists of a high degree of sensitivity to prompts of low degrees of explicitness. The "skill" that increases across the age range shown is a series of progressive decreases in the threshold for triggering the elaboration process. The research suggests that this skill is a reasonable one to acquire: virtually all people appear to acquire it to a considerable degree and many people appear to acquire it completely. Furthermore, it is to considerable assistance on this kind of memory task, a task that is relatively common even for adults. So the questions are: When should formal instruction in the skill be offered, and What should be the aim of instruction when it is offered?

My suggestion is that the answers to these questions about skill learning should be similar to the conclusions offered about content learning. The aim of instruction in skills should be optimal performance, that is mastery of the skill. The timing of instruction should be chosen so as to insure that virtually all children can attain mastery with relative ease and efficiency. This second stricture requires a modification of the first: even though the ultimate aim of skill instruction may be complete mastery of the skill, at any given age the immediate aim may be far short of mastery at a fully mature level. So, for example, the appropriate aim of instruction in elaboration skills at age seven would be to assist the child in gaining sensitivity to moderate prompts, not to free him of the need for explicit prompts of any kind.

With regard to skill instruction, my contention is that the results of experimental-developmental research offer guidance both as to the timing of instruction, the immediate aims of instruction, and the ultimate aims of instruction. The question of ultimate aims can be disposed of most easily. The ultimate aims of skill instruction should be to assist students in mastering those useful skills that substantial numbers of the adult population eventually achieve. In the case of the elaboration process, it appears that a substantial portion of the population acquire the skill of optimal performance in the presence of only minimally explicit prompts.

A Rule of Thumb for the Timing of Instruction

Starting with that ultimate aim, the strategy is to work backwards. Thus, the next question is: when should formal instruction designed to foster this aim begin? Let me propose a rule of thumb—instruction should begin (a) only after the immediately prior skill has been mastered, and (b) when the developmental curve for the skill shows signs of rising. In other words, formal instruction should be offered to children when they are in a transition period with respect to the particular skill we wish to foster. This rule can be explicated with reference to the charts in Figure 2. The schematic curves in each panel represent increases with age

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Insert Figure 2 about here

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in sensitivity of the elaboration process to the four types of prompts. An inspection of the figure indicates that the optimal time for introducing instruction in the skill of using minimal prompts is bracketed by the age range of 12 to 15 years. In this range students have mastered the skill of using moderate prompts to achieve optimal performance on noun-pair tasks. It is also clear that by the middle of the range, the developmental curve for a substantial number of students (upper panel) shows a sharp deflection relative to the previous age range. This deflection is the cue for introducing instruction, both for high-SES and for low-SES students.

Working backwards again, the skill immediately preceding the ultimate one is that of deriving full advantage from moderate prompts. Using the proposed rule of thumb, it appears that instruction in this skill should begin at approximately age 7. Extrapolating from the figure, it can be guessed that by then, many children have fully mastered the skill of using substantial prompts, and that the curve for moderate prompts has begun to rise. Similarly, an application of the rule with respect to the use of substantial prompts suggests that instruction begin at about age 6.

Some of our research illustrates the consequences of ignoring this rule of thumb. In one study (Rohwer & Ammon, 1971) we offered formal instruction to seven year olds in the skill of performing optimally in the presence of minimal prompts. Although we detected statistically significant effects of instruction, the results were disappointing: the children did not come close to attaining optimal performance levels. Later, we compounded our error by offering a similar kind of instruction to four and five year olds (Rohwer, Ammon & Levin, 1971). Although we improved the instructional materials and procedures over those used in the initial study, the aim was still the same--to equip the children to perform optimally given prompts that were only minimally explicit. The results showed that the two weeks of instruction were of no effect whatever. With hindsight--it is an understatement to say that our timing was bad--we should have been working with 13 or 14 year olds. Thus, the proposed rule of thumb about the timing and aims of instruction is not without evidential support.

Consider how the proposed rule of thumb can be applied to derive hypotheses about the timing and aims of instruction in school-subject skills. Use of the rule depends on knowing five things: (a) the conditions necessary for optimal performance on the task of interest; (b) the character of the underlying process responsible for performance on the task; (c) the extent to which a substantial majority of adults possess the skill of activating the process; (d) the major landmarks along the way to achieving the final form of the skill; and (e) knowing the form of the developmental function so as to identify the ages at which the achievement of each landmark can be accomplished with ease and efficiency. At present, however, these prerequisites to successful application of the rule are not available for school subjects.

A strategy for gaining the information prerequisite to decisive research has been illustrated in terms of the case study. The strategy could be further improved by analyzing school-subject tasks to reveal their essential psychological components, creating non-school tasks that are isomorphic with these components, and conducting coherent programs of research using the new tasks. The reason for insisting on non-school tasks, of course, is that school-subject tasks cannot be used in such research until the present lock on the timing of their introduction is opened. And that lock can not be opened until we know enough to make a key.

In conclusion, I want to emphasize my agreement with Bereiter's dictum: schools should teach only what is teachable (Bereiter, 1969). But, the only way to find out what is teachable is by discovering how and when it can be learned with ease. Some may disagree with these dicta, believing that learning is not learning unless it hurts. But, I know of no evidence, and can draw on no experience, to contradict the conviction that failing to learn mainly teaches us to hate learning, and in the process, how to fail.

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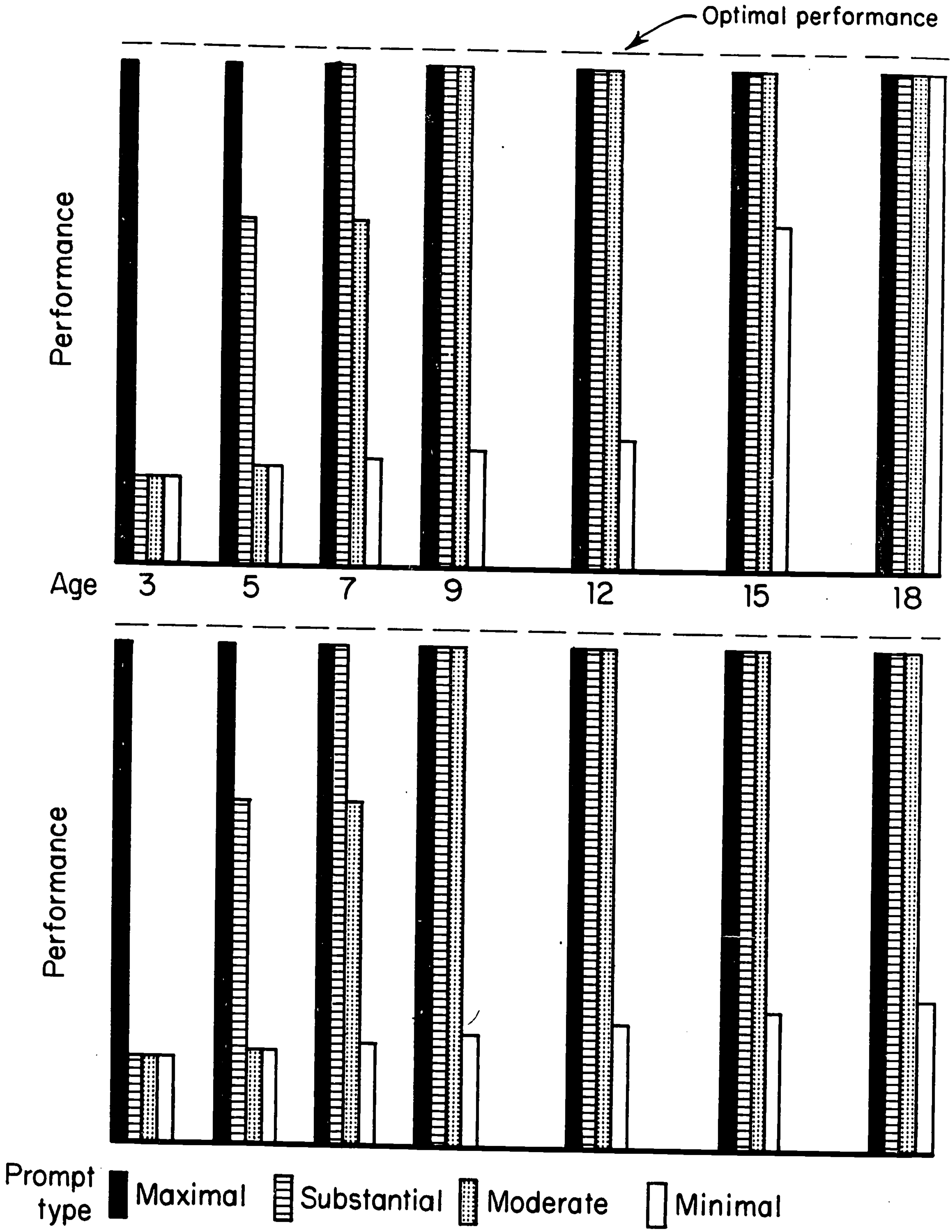
Footnote

[1] Invited address presented at the annual meeting of the American Educational Research Association, Chicago, April, 1972.

Figure Captions

Figure 1. Summary of prompt types required to achieve optimal performance as a function of age. Schematic patterns for high-SES (upper panel) and low-SES (lower panel) populations.

Figure 2. Hypothetical course of developmental changes in sensitivity to four kinds of elaborative prompts: upper panel--high-SES populations; lower panel--low-SES populations.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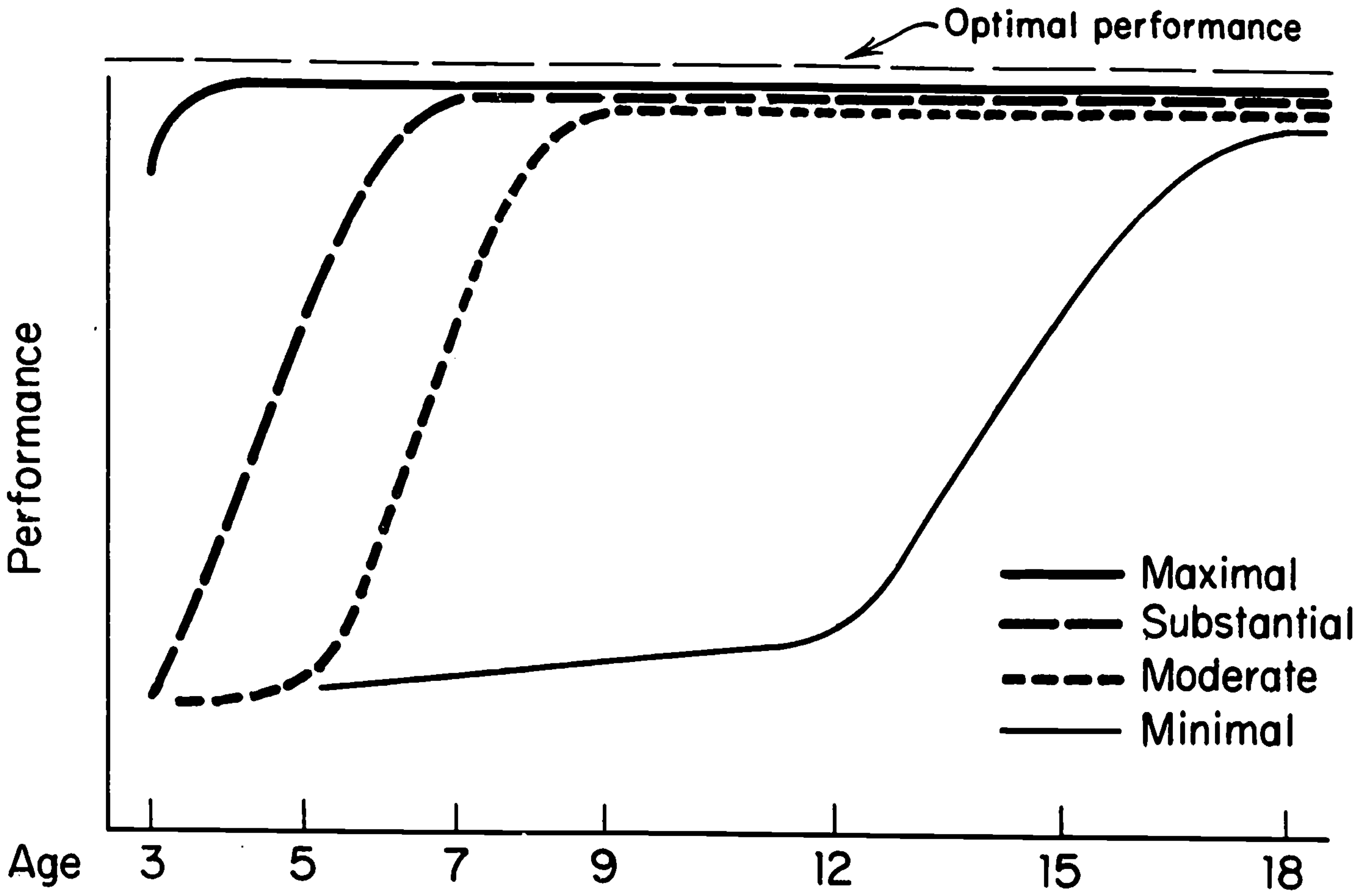

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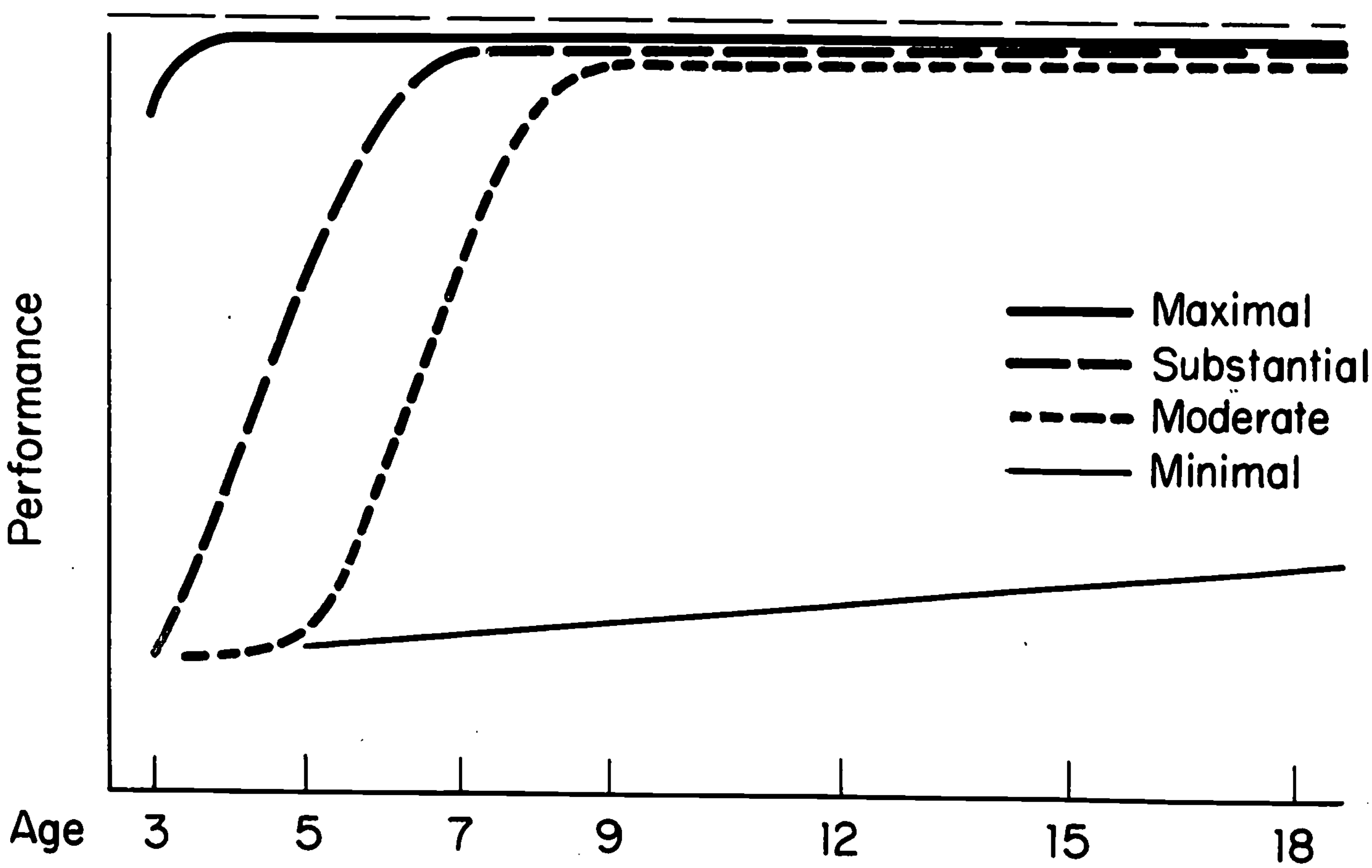

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